

# A VIVISECTION CONTROVERSY

Being a discussion on certain  
Medical Theories and Statistics  
— carried on in the —  
"CHELTENHAM EXAMINER"  
during  
December, 1910, & January, 1911.

BETWEEN

**WALTER R. HADWEN,**

J.P., M.D., L.R.C.P., M.R.C.S., etc.

*(President of the British Union for Abolition of Vivisection),*

AND

**STEPHEN PAGET, Esq.,**

F.R.C.S.

*(Hon. Sec. of the Research Defence Society).*

PUBLISHED BY

**The British Union for Abolition of Vivisection**  
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# A Vivisection Controversy.

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The reader who will patiently peruse and study the following letters will find himself rewarded, not only by the acquisition of certain facts, but by the insight which he will thus obtain into the respective methods in controversy of the Vivisectionists and Anti-vivisectionists. They are specially commended to the attention of those who take an interest in the abstract study of debate. The only remark that needs to be made upon them is that they are typical samples of the Vivisection controversy, similar (on one side identical) letters having appeared in various organs of the press throughout the country during the past three years.

As an introduction, the report of the meeting which formed the basis of the correspondence, as published in the *Cheltenham Examiner*, is given.

*From the "CHELTENHAM EXAMINER," Thursday, Dec. 1st, 1910.*

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## ANTI-VIVISECTION.

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### MEETING OF THE CHELTENHAM BRANCH OF THE BRITISH UNION.

**T**HE annual meeting of the Cheltenham and District Branch of the British Union for the Abolition of Vivisection was held at the Imperial Rooms on Tuesday afternoon, Dr. E. W. Wilkins, M.A., LL.D., in the chair.

#### **THE CHAIRMAN'S ADDRESS.**

The CHAIRMAN remarked that they had met to renew their warfare against a great evil, which was man-made and preventible, but which, owing to apathy and pre-occupation, was not fully realised by one in a hundred. His own feeling was that there was one great principle of the universe and of human life upon which there was very large agreement amongst all thoughtful people: poets, philosophers, advanced thinkers, and the seers of the world had recognised and taught that principle, which was that good, perfection, beauty, harmony, health, peace, order was the goal, the "final cause" of the universe and of man, and that it was man's work, business, duty, joy to effect this, to bring it about. When he spoke of health, he meant health in the largest and truest sense, not merely of the physical body, but of the whole nature—health of the soul and the well-being of the whole human race in every respect.

During the last twenty or thirty years the best ideals of medical science and the art of healing had been sullied by the adoption by a large section of the leaders of medical science of the most detestable practices, which only needed to be thought about in the light of the results obtained in other regions of knowledge to be recognised as not leading to true health, and therefore to be rejected as false science. That was the body of practices known as vivisection.

#### **UNWORTHY OF HUMANITY.**

Those practices fell under two heads—(1) the dissection of living animals, often with prolonged agony, and (2) the system and practice of deliberately implanting disease into living animals, either with a view to watching its progress, often extended over

periods of weeks and months, or with a view to using the body of the animal as a kind of laboratory to elaborate diseased fluids, to draw off these diseased fluids or serums, and then after various cultures and attenuations to inject them into human bodies with a view to mitigating or preventing specific diseases.

After quoting, as an illustration of what he meant, a description from the *Contemporary Review* of August, 1909, of Colonel Bruce's investigations into the origin of Malta fever, he observed that, whatever beneficial results might be claimed from such practices as were used towards 150 or 200 monkeys on that occasion, they were utterly unworthy of our humanity. Those whose aim was to live at their best should refuse and reject such gifts or benefits unhesitatingly. Health could never come that way. They could not have a healthy body by poisoning their soul—that was, all the higher principles of their nature. Health in the true sense included much more than freedom from bodily disease. The great mistake made by the medical scientists who pursued the path of vivisection was that they subordinated regard for the moral purpose of the universe—the divine order, beauty, harmony, love and perfection which constituted the true end and purpose of the universe—to the study of material forms and the order of the mechanical processes of nature. It was on these grounds, apart from his firm belief that no real bodily health resulted from the injection into human beings of diseased animal fluids obtained in the way described, that he should for ever refuse to participate in their use. Prolongation of physical life or immunity from disease, even if it could be procured, ought not to be purchased on such terms.

### DR. HADWEN'S SPEECH.

DR. W. R. HADWEN, President of the British Union, gave an address on "Experiments on living animals." There was no question, he remarked, that such experiments ran directly counter to that high ideal of human life which Dr. Wilkins had put before them. Vivisection was an absolutely erroneous practice, because it dealt almost entirely with results instead of with causes. The anatomic and physiological characteristics of animals were, in practically every instance, quite diverse from those of the human body, besides which it was illogical to suppose that they could arrive at any scientific result from experimenting with human disease upon a healthy animal. The injection of a human malady into an animal rarely produced the same disease in the animal; therefore no conclusion therefrom could be drawn for the benefit of the human race.

So far from vivisection having produced any result, it had been nothing else from beginning to the end than a gigantic failure. He did not know that they could have a better illustration of this wrong-headedness, this unscientific method, than the recent onset upon rats in Suffolk. Four persons in one house were taken ill with the same complaint, viz., pneumonia. So far as he could gather, there was no other symptom in the patients than inflammation of the lungs; but fear was in the air, somebody whispered "Plague," word was sent to the Local Government Board that most likely rats were the cause of the plague, and some dead rats were



sent to the Board for examination. The bacteriologists thereupon set to work and discovered in the rats what they considered to be the plague microbe, word went forth that rats must be destroyed, and up to the present thousands upon thousands of rats had been got rid of.

Assuming for the sake of argument that the rats had suffered from plague, what was plague? There was no trustworthy evidence that he, as a student of epidemiology, had ever heard of that plague had ever come to a human being by means either of a rat or a rat flea.

### THE GERM THEORY.

The plague of mediæval times and of the East arose from soil poisoning—it was due to cadaveric deposit, the deposit of dead bodies, or from filth of some kind or another emanating from the soil. To say that plague arose from one definite cause which had never altered, and that that cause was a microbe, was about the most absurd idea that had ever originated in the human brain. It arose in this country, in the middle ages, for the most part in walled cities, in monasteries, and in the houses of the clergy. Why? Because in these places the soil had become poisoned by a larger cadaveric deposit than it could make use of and render immune. If they were to get rid of any disease, the first thing was to find out what had produced it and to remove the cause. The bacteriologists, however, never troubled to find the origin of disease: what they tried to do, as they did in the case of the so-called rat plague, was to try and find a microbe.

In every case of plague, whether in England, in India, in China, or other parts of the East, it was officially recorded that rats always gave the premonitory signs of the coming epidemic. That was easily explained. In soil poisoning the smallest animals were affected first; then came goats, pigs, and fowls, until at last the disease reached the human being. What, then, should have been done in East Anglia, when the rats, as was alleged, gave a warning of this description? Was it not to have found out what was the nature of the poison by which the rats had become affected? He suggested various sources of soil pollution, and ridiculed the notion that the "plague" could be got rid of simply by destroying vermin which had given warning of the existence of filth. It was just as futile, he suggested, to search for the germ.

The germ theory embraced the grossest cruelty to animals. Yet he emphatically declared that this theory—the theory that every kind of zymotic disease had its origin in a certain microbe, and that every specific disease had its own specific microbe—had never yet been proved. Every attempt at proving it had ended in failure. It was M. Pasteur who invented the germ theory. Professor Koch had put forward four postulates which must be proved before the theory could be accepted as scientific, but every one of these postulates had broken down in every disease to which they had been applied. If they had no scientific foundation for the germ theory what, then, became of the superstructure built upon it? It crumbled to pieces. Koch had admitted, "You cannot argue from animals to man."

### THE GERM CARRIER.

Another illustration of the point was the recent case of a so-called germ carrier at Wrexham. 107 persons were taken ill and five

died after eating pork pies. All the symptoms pointed to ptomaine poisoning, but in order to introduce the new-fangled theory of a germ-carrier, the Medical Officer of Health made a bacteriological examination of the blood, &c., of all employed in the bakery where the pork pies were manufactured, and fixed upon a widow who, it appeared, had worked at the shop for seven years before and for eight weeks after the outbreak, yet had neither before nor since been the cause of any mischief.

But the extraordinary part of the whole business was that the supposed germ-carrier had never herself suffered from paratyphoid enteritidis—the disease from which the 107 pork-pie eaters were said to have suffered—nor had she touched any one of the pies which were alleged to have caused the illness, nor yet had she infected her own children who lived with her. When science so-called had reached this position, based as it was upon the experimentation upon living animals, it deserved the condemnation of every common sense man in the kingdom; and it seemed to him if things went on much longer in this way, that the general public would very soon have to rise up and save the whole medical profession from the lunatic asylum. The speaker gave a further illustration of his contention in what had happened in the search for a germ for sleeping sickness.

### **PHYSIOLOGICAL CONTRADICTIONS.**

Turning to the physiological aspect of vivisection, he described “the diabolical cruelty” which was practised on dogs in order to study digestion and the action of gastric juices, and quoted Professor Starling’s evidence before the Royal Commission to the effect that “the last experiment must be upon man.” If the last experiment be upon man, it was perfectly clear that the first scientific experiment must also be upon man, and if so, then all the previous experiments upon lower animals had been unreliable and had produced no scientific result. A hedgehog could swallow as much opium as would last a Chinaman a fortnight, and as much prussic acid as would kill a regiment of soldiers; long-tailed monkeys could take large quantities of strychnine without effect, while short-tailed monkeys were killed with a very small dose; and a little pigeon could take twelve grains of morphia and fly away as happy as a lark, while a single grain would send any of those present to their last account. From every standpoint vivisection was absolutely misleading. Lawson Tait had declared that in surgery experiments on animals had done nothing for the human subject except to lead to horrible bungling, while in the matter of therapeutics the whole work of the vivisector had been nothing more nor less than a gigantic failure. Furthermore, it was one of the foulest blots upon a Christian country, and was diametrically opposed to the morality which a Christian nation professed to uphold.



# THE CONTROVERSY.

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*From the "CHELTENHAM EXAMINER," Dec. 8th, 1910.*

## **Research Defence Society.**

Sir,—At a recent Anti-vivisection meeting in Cheltenham, of which you gave an account in the *Cheltenham Examiner*, December 1st, reference was made to three diseases, plague, Malta fever, and sleeping sickness. Give me leave to put before your readers, briefly, certain facts, which cannot be denied, as to the cause and the preventive treatment of the first of these diseases.

Plague is not due to any sort or kind of soil poisoning, cadaveric deposit, or filth. Such phrases are just a sort of popular talk; they have no exact meaning of any sort or kind. Plague is due to a special germ which was discovered in 1894, by Kitasato and Yersin. In 1895, it was found possible to immunise guinea pigs and rabbits against plague. In 1897, Haffkine and others, having tried this treatment on themselves, introduced it into practice. The evidence that plague is due to a special sort of germ, and to no other cause of any kind, and that these germs are in the blood of rats infected with plague, and in the stomachs of the fleas on the rats, is past all possibility of reasonable doubt. When the rats die, the fleas leave them, and seek live blood elsewhere: and that is how the plague is conveyed to man. The Suffolk patients died, not of ordinary pneumonia, but of plague *plus* pneumonia: and the germs of plague were found in them during life. These facts speak for themselves. I come now to the results obtained by preventive treatment against plague in India.

In Undhera, an agricultural village in Baroda State, of 950 inhabitants, 513 were protected, and the rest were not. Plague occurred, subsequently, in 28 families. In these families there were 71 protected, and 64 not protected. The 71 protected had eight cases with three deaths, while the 64 not protected had 27 cases with two deaths.

In a tenement dwelling at Pilat Bandar, Bombay, were 24 protected persons, and 37 not protected. Of the 24 protected, one had plague and recovered: of the 37 not protected, 19 had plague and 12 died.

In the Army Hospital Corps, Belgaum, were 83 persons living close to the European Military Hospital under constant supervision of these 83 persons. 80 were protected, and only three were not protected. Of the three not protected, two died of plague: none of the 80 suffered in any way.

The Senior Medical Officer, Belgaum, had, among his private

servants and their families, 28 protected, and only two not protected. These two persons both died ; but none of the 28 got the plague.

At Hubli, among the railway employés, 1260 were protected, and 760 were not protected. Among the 1,260, only two died ; among the 760, there were 21 deaths.

At the Empress Mills, Nagpur, 1,116 factory-hands were protected, and 2,663 not protected. Among the protected, six died of plague : among the not protected, 179 died of plague.

In Bombay, a large number of municipal servants were protected. The municipal servants occupied special municipal tenements, and were therefore under easy daily observation. Among 3,317 protected, only three died of plague : among 838 not protected, 18 died of plague.

In Karachi, nearly all the town scavengers were protected. Among no less than 1,245 protected, only four died of plague : among only 60 not protected, five died.

In the Punjaub villages, inoculation is carried out vigorously. We have the figures large enough to satisfy anybody. We know what happened among 186,797 protected persons, and 639,630 not protected. Among the protected, only 314 died of plague. Among the not protected, 29,723 died of plague. That is to say, we may roughly calculate that 8,000 human lives were saved by preventive treatment against plague in the Punjaub villages.

Let me add, Sir, that I shall be happy to send, to any of your readers who will send me seven stamps, an illustrated pamphlet on *Plague in India, past and present*, written by Lt.-Colonel Bannerman, I.M.S., D.Sc., M.D., Director of the Bombay Bacteriological Laboratory. It gives an admirable account of the experiments which made it clear that rat-fleas convey plague from rats to man : it also describes the whole nature and method of the preventive treatment. I hope that you will allow me next week to say something about Malta fever.—

I remain, yours, etc.,

STEPHEN PAGET,

Hon. Secretary, Research Defence Society.

21 Ladbroke Square,

London, W., Dec. 3, 1910.

*From the "CHELTENHAM EXAMINER," Dec. 15, 1910.*

## **Dr. Hadwen's Reply to Mr. Paget.**

### **PLAGUE.**

Sir,—Mr. Stephen Paget is a past master in the art of assertion and denial when he has to deal with questions which he has never adequately studied. The presumption and assumption with which he waives the whole epidemiological history of plague as recognised by every intelligent student of the subject, and of which I afforded ample evidence in my lecture, is ludicrous.

He says the four patients in the little Suffolk village about whom all this artificial and ridiculous scare has been created, died of plague *plus* pneumonia. When he can show that they had any physical sign or symptom other than that of ordinary pneumonia, it will be time enough for him to make this assertion.

The President of the Local Government Board, in answer to questions in the House of Commons, has never ventured beyond the statement that plague was "suspected." No "plague germs" were "found in them during life."

To the germ theory origin of plague with its rats and fleas and microbes, Mr. Paget bows. It is the latest fashion and it saves a lot of thinking. The bacteriologists have been very busy examining the Suffolk rats for some time, but I am not aware that they have been able to find a "plague" flea yet—the only fleas that they can find so far have always been declared to be of the harmless variety! The only time they came across a real "Cheops" was long ago down at Plymouth, but there was no plague! The "plague bacillus" which they have "discovered" has done duty already for nearly every disease under the sun, and will probably do duty for a good many more before they have finished with it. Bacteriologists are not certain which is the right one, for they found one bacillus in the blood of plague patients, and another in their buboes. Not even the Suffolk rat is genuine, for Sir Ray Lankester, who now says the brown rat ought to be extirpated, declared two years ago that it was quite harmless, and that owing to its having driven the black rat out of the country, England had been saved from the plague! The fact is, all the modern germ theorists are covering themselves with ridicule by their contradictions and fright.

Added to all this, I note that Mr. Hankin, Chemical Examiner and Bacteriologist for the N.W. Provinces of India and Oudh, reported officially to the Plague Commission, 1899, that there was no doubt that plague occurred among human beings in whom no microbes could be found at the time of death. Experiments have been made by putting the alleged plague flea on human beings, allowing them to bite, and even rubbing in the supposed poison, and yet no plague was the result.

As to all the instances which Mr. Paget gives of the wonderful effects of Haffkine's plague serum (a subject to which I did not refer in my lecture), he surely ought to know by this time that Haffkine's fluid has been given up as useless, and that therefore all his wonderful stories must be fictitious! Mr. Paget told a similar remarkable tale about Yellow Fever serum with precise data and statistics in his first edition of "Experiments on animals," but he had to promptly withdraw it all in the second edition; he did so without even an apology or an explanation for having so flagrantly imposed upon his readers. All Mr. Paget's miracles had been wrought by the wrong germ! And they have not yet been able to find the right one!

Moreover there is a grave fallacy associated with all Mr. Paget's figures. In the first place the differences in the fatality between the inoculated and the uninoculated cases are for the most part slight and are easily accounted for by the fact that the inoculated cases are more or less selected, whereas the uninoculated include all the rapidly fatal cases. The statistics are therefore unscientific.

In addition to this, no account is taken of the character of dwelling place in which the sufferers lived. It has been conclusively shown that the type of house—whether of stone with stone flooring or mud walls and a mud floor—makes all the difference to the attack rate.

Look at India as a whole. The plague began in 1896 and claimed 30,000 deaths in the *year*. The bacteriologists commenced the wholesale distribution of serum in 1897, and by the year 1904 the deaths had reached 30,000 per *week*. They increased every year for nine years (except in 1900). From September, 1906, to April 1907, there were 5,326,000 deaths. That does not say much for serum. It only increased the mortality. In 1908 the heavy rains washed the soil and the slump in plague was sudden; it was almost extinguished. But directly the heat played again on the rotting soil, cholera (another soil poison), and small-pox (an aerial poison), took its place for a time and produced more deaths than the plague. Now plague has come back. And plague will continue until medical officialdom is stopped in its insane process of blood poisoning and plague dissemination, and the work of the Sanitary Engineer is called into requisition instead.

Plague broke out in Alexandria in 1899. Dr. Rogers, Director-General of the Sanitary Department, Egypt, "dealt with it on lines directly opposed to Mr. Haffkine's views."<sup>1</sup> He scorned the inoculation, just as Haffkine scorns sanitation. He stamped out the plague in Alexandria in seven months.

I am, Sir, your obedient Servant,

WALTER R. HADWEN, M.D., J.P.

President of the British Union for the Abolition  
of Vivisection,

Gloucester, December 12th, 1910.

*From the "CHELTENHAM EXAMINER," Dec. 15, 1910.*

### **Research Defence Society.**

Sir,—Malta fever is another good instance of the value and the necessity of experiments on animals. This painful, slow, exhausting, and often fatal disease used to be the very scourge of our garrison in Malta. The average stay in hospital, for a case of Malta fever, was four months. In one year alone, 1905, no less than 403 officers and men were invalided home, as the result of Malta fever.

Sir David Bruce, as far back as 1887, discovered the special germ which is the cause of the disease. He made the discovery, of course, by the only possible way, by experiments on animals. First, he found certain germs, under the microscope, in the blood and tissues of patients who had died of Malta fever. Then he cultivated these germs, all by themselves, in test tubes apart from all other kinds of germs. Then he introduced a little of this "pure culture" under the skin of monkeys. These monkeys then showed the usual signs of Malta fever: and when they died, or were killed, the germs were found in great numbers in their blood and tissues.

If anybody thinks that Malta fever is due to any other cause than these germs, he must explain how it is that several of our men of science, in English laboratories, hundreds of miles from Malta, working with pure cultures of these germs, have become infected with Malta fever. There have been seven or eight cases of this kind of infection, and one of them ended in death.

Between 1887 and 1904, some 14,000 or 15,000 of our sailors and soldiers were stricken with the disease. At last, in 1904, our Government sent out a Commission, organised by the Royal Society. After much laborious work and many experiments, this Commission decided that the disease was not conveyed through the air, nor through the water supply, nor by pollution of sewage, nor by contact: nor was it conveyed, like malaria, yellow fever, sleeping sickness, and simple continued fever, by the bites of insects. Finally, the Commission discovered that Malta fever might be conveyed by food; for they added a little of a pure culture of the germs to the food of some monkeys, and all these monkeys developed the disease. Thus, Bruce and his colleagues were led to examine the food supply of the garrison.

They examined, of course, the goats: because goat's milk is commonly drunk all over the island. The goats themselves were healthy enough, but many of them, when they were tested, showed signs of having the germs in their blood, or even in their milk. Therefore, the Commission tested some thousands of the goats. And they found, to their great surprise, that quite 50 per cent. of them were carrying the germs in their blood, and 10 per cent. were actually yielding the poison of the fever in their milk. Monkeys fed on milk from one of these goats, even for one day, almost invariably got the disease.



It is to be noted that, in years past, our officers, with their wives and children, had suffered more than three times more heavily than the non-commissioned officers and men. The officers and their families, of course, took more of the milk. If Malta fever had been due to the unwholesome situation of the garrison, or to the bad drainage of the harbour, then the men would have suffered more heavily than the officers : for the men were crowded together in stuffy barracks, but the officers lived in well-ventilated houses.

About this time the ss. *Joshua Nicholson* shipped 65 goats at Malta for export to America. There were 27 persons on board, between Malta and Antwerp, where the ship touched ; and, of course, the goats' milk was drunk on the voyage. Of these 27 persons, 17 have been traced. Of these 27 persons, two always boiled the milk, and thus were protected. Of the remaining 15, no less than five contracted Malta fever. The goats were landed in America, put in quarantine, and carefully watched. A woman, living on the quarantine station, who took the milk, got the fever and died of it. One by one, as time went on, the goats all showed signs of severe infection, and all had to be killed.

In Malta, early in 1906, our men were beginning to be warned against the goats' milk ; and on July 1st, 1906, the official order was issued, forbidding the milk to be supplied to the garrison.

Now for the result of that order. Sanitary improvements may have checked, but certainly had not stopped, the disease. In 1905, there had been 643 cases. In 1906, up to July 1st, there had been 123 cases. During the rest of 1906, including July, August, and September, always the worst months for the disease, there were only 40 cases. In 1907, there were 11 cases. In 1908, there were five. In 1909, there was one. In 1910, there have been none.

The people of Malta, long after the garrison was protected, went on drinking the milk, and having the fever. It remains to discover how the goats themselves become infected, and a Commission was recently appointed to study this particular point.

I shall be happy, sir, to send any of your readers, who will send me a postcard, a fuller account of these facts. There has been no criticism of them that needs any notice ; and they are a striking example of the absolute necessity of experiments on animals.

I remain, etc.,

STEPHEN PAGET,

Hon. Sec., Research Defence Society.

21 Ladbroke Square, London, W.

Dec. 10, 1910.



From the "*CHELTENHAM EXAMINER*," Dec. 22, 1910.

## Dr. Hadwen's Reply to Mr. Paget.

### MALTA FEVER.

Sir,—In my criticism of Mr. Paget's letter upon plague, I proved him to be guilty of culpable ignorance. I now charge him, in regard to Malta Fever, with gross and reckless misrepresentations. He says, "there has been no criticism of his 'facts' that needs any notice," but I perceive he has nevertheless very adroitly so trimmed his letter of Dec. 15th, as to evade, if possible, the criticism and exposure to which I have submitted the false statements he previously published upon the same subject.

(1) He begins by exaggerating the character of Malta Fever as a disease which is "often fatal." Its mortality has never exceeded two per cent.

(2) He proceeds to write a long rigmarole upon the discovery of the "Malta Fever germ" by Colonel Bruce in 1887. As a matter of fact, Dr. Fazeo, of Naples, had declared eight years before that in his opinion it was due to a micrococcus. Colonel (then Captain) Bruce concluded that one out of many microbes he came across in the spleen of a dead soldier was the identical germ. By injecting a cultivation of it into the bodies of various animals he produced symptoms of blood poisoning, called it Malta Fever, and alleged upon the basis of Koch's postulates that he had proved his case. I have shown conclusively in my articles in the *Contemporary Review* of August and November, 1909, that every one of Koch's postulates has been falsified and that, therefore, the *micrococcus melitensis* is a fraud, and I challenge Mr. Paget to disprove my conclusions.

(3) That germs will be found in the bodies of dead monkeys if they have been previously injected into them is no matter of surprise, but that does not prove that the germs were the origin of Malta Fever. A rise in temperature in monkeys, who cannot express their symptoms, after being injected with morbid material, is only an evidence of blood poisoning, and the cruel conditions under which these poor creatures, brought fresh from their forest homes, were kept and treated in the cramped up cages which I saw on the roof of the Health Department at Malta, were enough to account for any feverish illness, apart from the injection.

(4) Mr. Paget asks for an explanation as to why men working in English laboratories with cultures of the alleged Malta Fever germ contracted Malta Fever if the germ was not a genuine one. I believe there have been two authentic cases only, one of which died. They broke a tube containing this morbid material and injured themselves. From the enquiries I have made as to the symptoms, they appear to have been pure cases of *septicæmia*. Any filth of similar character would have produced the same result.

(5) Mr. Paget says that the Commission which went out, after "much laborious work and many experiments, discovered that Malta Fever was conveyed by food." This is untrue. After two years of the most atrocious and unscientific experimentation they confessed that they had discovered nothing, and they were at their wits' end to account for the source of the mischief until they heard of a wild and exaggerated story of an American cargo boat; it was from this fairy tale that the Commissioners obtained their "science," or, to use Colonel Bruce's own words, it "clinched the fact." The whole of the "many experiments" in regard to man are limited to this one solitary fictitious incident.

(6) In spite of the fact that I gave the full and authentic details of this ridiculous story in my *Contemporary* articles, and although Mr. Paget's attention has been drawn to the facts repeatedly since, he even now has the audacity to repeat his mis-statements, with the following exception:—he cautiously accepts my evidence that only five men were ill on board the *Joshua Nicholson*, which sailed from Malta with 65 goats on board, but evades the truth by saying they were "shipped for export to America," and instead of honestly saying the *Joshua Nicholson* did not take them to America at all, he says "there were 27 persons on board between Malta and Antwerp." All this is to try to escape my criticism by which I have exposed his misrepresentations.

The facts are these: Colonel Bruce told the Royal Commission that "broadly every man on board who drank the milk took Malta Fever," and as the result of his assertions and his professed discovery of the goats' milk origin of Malta Fever, he received a knighthood. What I have proved is this, that the *Joshua Nicholson* with 27 persons on board only went as far as Antwerp, that the 65 goats were transhipped on board the *St. Andrew* at Antwerp for New York with 65 persons on board; that besides this total of 90 persons—all of whom drank the milk of the goats—large numbers drank of the milk while the goats were in quarantine at Antwerp, and again at New York, and yet out of this large number, only five persons can be shown to have been ill. There is no evidence that these five had Malta Fever beyond a fallacious laboratory test applied to their blood after they had fully recovered. From the clinical evidence it is highly probable it was not Malta Fever at all, probably only sea sickness. No medical man saw them; and yet out of this wild story, and Colonel Bruce's false statements, the whole goats' milk theory has been built up. And although I have compelled Mr. Paget to admit the falsity of some of his statements, he still goes on publishing his impudent pamphlet on the subject.

(7) He says a woman living on the quarantine station who took the milk, got the fever and died. There is no evidence that she died of Malta Fever. Even if she did, she had drunk bacteriologically pure milk for three months. Mr. Paget's treasurer, Dr. Sandwith, actually said at Brighton a fortnight ago, that this woman's husband died of Malta Fever also. The statement is false. He died of "bilateral pneumonia following influenza," and there is no evidence at all that he ever had Malta Fever. Mr. Paget says the goats were killed as they "showed signs of severe infection." This again is false: from first to last the goats showed no sign of infection, they were as healthy as they were the day they left Malta. And as for the *micrococcus melitensis*

which the bacteriologists allege to have found in their milk, the Chief Analyst in the Health Office at Malta confessed, after I had carefully examined these germs and had observed that they looked uncommonly like fat globules, that it was very difficult indeed to tell the difference. I have learnt by experience that a bacteriologist with a microscope on the one hand, and a vivid imagination upon the other, can prove anything he wants to.

(8) Mr. Paget winds up with statistics which he treats dishonestly. He says "the men were beginning to be warned against the goats' milk early in 1906." He says this to evade the crushing indictment I have brought against him that the milk was not stopped until July 1st. Colonel Bruce himself admitted under cross-examination that *no* remedial measures were taken until July 1st, and yet before ever the goats' milk was stopped to the troops, the cases had suddenly dropped down from 643 to 120. Why? Stopping the milk on July 1st could not stop Malta Fever six months previously. The fact was at the end of 1905 the soldiers had been hastily removed from the horrible insanitary St. Elmo barracks, where nearly all the Malta Fever was occurring, to new and splendid barracks upon the hill side.

(9) Mr. Paget says nothing about the Navy. The statistics are not good enough for his purpose. I have shown that Malta Fever has been gradually declining in the Navy at Malta ever since the harbour has been in the process of cleansing from its centuries of sewage deposit.

(10) But, perhaps, the most inexcusable assertion in Mr. Paget's letter—which really amounts to a subterfuge—is that "the people of Malta, long after the garrison was protected, went on drinking the milk and having the fever." The fact is that the civil population has never had an attack rate of more than four per 1,000. The military have had an attack rate ten times as great and the naval five times as great, due to the shocking condition of the barracks in the former, and the harbour in the latter. Since the goats' milk was stopped to the garrison, the civil population have drunk more goats' milk than ever, and yet the attack rate from fever has gone down 50 per cent. What little Malta Fever there is among the garrison is now called "Simple Continued Fever" instead.

When I was in Malta investigating this question, I drank freely, as likewise did my daughter, of goats' milk, and I am prepared to spend my next holidays there and to drink freely of any goats' milk which the public analyst shall certify to contain the *micrococcus melitensis*.

The military authorities and their goats' milk theory have become the laughing stock of all the inhabitants on the island.

Yours, etc.,

WALTER R. HADWEN, M.D., J.P.

Gloucester, Dec. 19th, 1910.

From the "*CHELTENHAM EXAMINER*," Dec. 22, 1910.

### Research Defence Society.

Sir,—in reply to Dr. Hadwen, let me say (1), the germs of plague were found in the Suffolk cases ; (2) his statement about these germs is not true ; (3) Haffkine's serum, as he calls it, is not a serum at all ; (4) it has not been given up ; (5) it is useless to talk about soil poisons and aerial poison ; (6) he argues from Alexandria to India ; he might as well argue from Suffolk to India.

Let me now give your readers another striking instance of the value of experiments on animals : the results of the preventive treatment against typhoid fever, and against lockjaw.

We shall all be agreed that the cause of typhoid, and the true cause of lock-jaw, were discovered and proved by means of experiments on animals, and could not have been discovered, nor proved, without these experiments. And we all shall be agreed, that the preventive treatment against these two diseases also is due to experiments on animals, and would have been utterly impossible without these experiments.

The preventive treatment against typhoid may be judged by the statement published in the *Journal of the Royal Army Medical Corps*, February, 1909. "Sir W. B. Leishman published the results of anti-typhoid inoculation in the Army up to June 1st, 1908. The total number of men inoculated was 5,473, amongst whom 21 cases (3.8 per 1,000) and 2 deaths occurred. The number of non-inoculated was 6,610, with 187 cases, (28.3 per 1,000) with 26 deaths. The case-mortality was 9.5 per cent. in the inoculated, and in the non-inoculated 13.8 per cent. Several of the regiments, however, were not exposed to the chances of enteric (typhoid) fever ; so that no cases occurred amongst either the inoculated or the non-inoculated. If these regiments are excluded, the incidence of enteric fever amongst the inoculated is 6.6 per 1,000, and amongst the non-inoculated 39.5 per 1,000."

The preventive treatment of tetanus (lock-jaw) may be judged by two sets of facts. Horses are very liable to the disease, and an outbreak of tetanus will run through stables, killing one horse after another. The power of tetanus-antitoxin to protect horses was demonstrated in 1897 by Nocard, then director of the Pasteur Institute at Lille. He demonstrated on 30 horses, that the disease could not be produced in them by inoculation, if, not later than three or four days after the inoculation, a dose of the antitoxin was given to them. On the strength of these experiments, he distributed the antitoxin to veterinary surgeons, in districts where tetanus was very common among the horses, to be administered before the performance of veterinary operations, or after the occurrence of accidental wounds. Of 2705 horses thus protected, only one contracted tetanus ; and that



one recovered. These observations were personally made, in tetanus-infected districts, at the request of veterinary surgeons, who had been losing every year many animals from the disease. During the period of their observation, no less than 259 cases of tetanus were observed among animals who had not received the antitoxin. Equally striking results have been observed in the United States.

We come now to the preventive treatment against tetanus in man. I pass over the Calcutta cases, and the Prague cases; they are striking, but the figures are not large. For large figures, we have those curious cases which occur in American cities after the 4th of July celebrations: the cases of lacerated wounds of the hands from squibs, rockets, toy-pistols, &c., These wounds frequently get infected with the germs of lock-jaw. "One of the wounds most commonly followed by lock-jaw is the blank-cartridge wound of the hand, common on the glorious 4th of July. The death-rate from these wounds is appalling. An active campaign has been conducted throughout the medical profession to reduce this mortality. All over the country, surgeons and medical journals have advised the injection of tetanus-antitoxin in every case of blank-cartridge wound. The American Medical Association has compiled statistics of 4th of July fatalities for the past six years. Six years ago, the 4th of July tetanus cases numbered 416. Then physicians began a more general use of antitoxin in all cases of blank-cartridge and cannon-cracker wounds. As a result, from 416 cases of tetanus in 1903, the number dropped to 105 cases in 1904, 104 cases in 1905, 89 cases in 1906, 73 cases in 1907, and 55 cases in 1908." (The Conquest of Disease through Animal Experimentation, Warbasse, New York, 1910).

When we think of the long weariness and danger of typhoid, and the swift horror and suffering of lock-jaw, we must be thankful that so many lives have already been safeguarded against these two diseases.

I remain, Sir, your obedient servant,

STEPHEN PAGET,

Hon. Secretary, Research Defence Society.

21 Ladbroke Square, London, W.

Dec. 20, 1910.

*From the "CHELTENHAM EXAMINER," Dec. 29th, 1910.*

## **Dr. Hadwen's reply to Mr. Paget.**

### **TYPHOID FEVER AND LOCKJAW.**

Sir,—Mr. Paget answers nothing, but denies everything, and then rambles on to something else—a very simple method of controversy. Let me briefly examine his six-lined retort to my letter on "Plague."

(1) He says, "the germs of plague were found in the Suffolk cases."

If he will examine the contents of his own intestine in the same way he will find he is suffering from "plague" likewise.

(2) He says my "statement about these germs is not true."

Then let him disprove it.

(3) He says "Haffkine's serum is not a serum at all."

That is only begging the question. I suppose Mr. Paget calls it a "vaccine." It certainly is not that either. I should prefer to call it a tube of caged filth, but I use the ordinary term.

(4) He says "it has not been given up."

If he will turn to Haffkine's own evidence before the Indian Plague Commission he will see that it has been abandoned. My facts and figures proving it to be a dismal failure remain untouched.

(5) He says "it is useless to talk about soil poisons and aerial poisons."

To a mind obsessed by "germs" such is probably the case. But when Mr. Paget comes to study epidemiological history instead of laboratory theories it will be time enough for him to talk in this cavalier fashion.

(6) He says I "argue from Alexandria to India, and that I might as well argue from Suffolk to India."

The strange thing is that this is precisely what Mr. Paget has done! His first letter about Suffolk plague was full of India and nothing else. But last week, when lecturing at Shrewsbury, he was tackled by a member of my Committee about the plague at Alexandria, and had to confess he knew nothing about it, and innocently asked his interrogator if she were quite sure it was not malaria!

Mr. Paget having settled the plague to his own satisfaction, proceeds with his own stereotyped remarks upon typhoid and tetanus—neither of which I discussed in my lecture.

The figures he quotes from the *Journal of the Royal Army Medical Corps* are of a very misleading description. Typhoid fever is a soil poison and results from the actual introduction of animal organic



matter, usually of an excremental type, into the alimentary canal. There is no evidence of its being epidemic in the ordinary sense of the word, and therefore to compare a few cases of attack with the thousands of soldiers who had never come in contact with the source of the poison is merely playing with figures.

The cases of attack quoted by Mr. Paget are very few, only 200 among 12,000 men. Before we talk of figures, we need to know how these 200 came to be affected, and why there were 187 in one case and only 21 in the other. We should find that the question of inoculation or non-inoculation had nothing at all to do with it, but that the whole question resolved itself into the *locality* where the detached soldiers had drunk probably of befouled water from some shallow wells. Again, the fatality depends upon the strength of the poison, the resisting powers of the patient, and the environment. The difference between the fatality of the inoculated and un-inoculated classes cited by Mr. Paget is not great, namely, in round figures, 10 per cent. of the former and 13 per cent. of the latter; it proves conclusively that the typhoid inoculation neither prevented the disease nor staved off death. The slight difference must have been due to other factors.

My argument is substantiated by the evidence of Dr. Melville, late Civil Surgeon to the Natal Field force, reported in the *British Medical Journal* of April 20th, 1901. He had 295 cases of typhoid fever under his care. Thirty were inoculated and 265 un-inoculated. The inoculated fatality was 6·67 and the un-inoculated was only 1·89. He found that "complications among the inoculated soldiers were more numerous, the duration of the fever longer, and the death-rate higher."

It will be noted that the inoculated deaths quoted by Mr. Paget were 50 per cent. greater than those of Dr. Melville, so if the typhoid serum be of any value, it must have seriously deteriorated in its "efficacy" since the South African War.

Then Mr. Paget goes on to tetanus (lockjaw). I notice he takes good care to say nothing about the *curative* properties of the anti-tetanic serum. That, alas, has had to go on the rubbish heap, with so many other of Mr. Paget's boasted wonders!

He begins about horses at Lille, and lumps up hundreds, inoculated and un-inoculated, without taking the least notice of their stables, their treatment, or their work. It is simple enough to distribute tubes of quackery among veterinary surgeons for their use on animals "before operations and after accidents," where every effort at cleanliness would at the same time be adopted, and then claim the serum as the saviour. Every Cheap Jack has worked the same trick for ages. Millions of horses have been operated upon without any serum injection and have recovered; and to compare such cases with other poor beasts which had no vet to look after them and no one to clean their wounds is an insult to ordinary intelligence.

Mr. Paget goes on to deal with tetanus in man, quoting the old threadbare story of the 4th of July celebrations in America. Nearly all of Mr. Paget's illustrations consist of "fairy tales from countries far over the sea," but if I dare to quote a foreign country, he holds me up to half a newspaper column of righteous indignation!

Now, all this farrago quoted by Mr. Paget was given by Sir Henry Morris, President of the Royal College of Surgeons, before the present

Vivisection Commission, and we have the benefit of his cross-examination—a benefit we cannot get out of Mr. Paget, for he makes his statements and then runs away, and when his “facts” are disproved and his figures are shown to be misleading he settles his opponents with blank denials of everything. But Sir Henry Morris, at close quarters, came off very badly, and had at last to confess (Q. 77.38) that we “could not arrive at any absolute conclusion from the figures.” That, therefore, knocks all Mr. Paget’s dogmatically-quoted figures on the head!

But more. What was proved was this—(1) That the evil results from tetanus depended largely upon whether the 4th of July was wet or dry; (2) There was no evidence that the serum was used in all the cases; (3) That the wounds are more speedily washed now than formerly, and that the serum has been getting the credit which belongs to the water-tap.

Finally, Sir Henry owned that he had himself tried the anti-tetanic serum on three cases and they all died!

When Dr. Martin, the Director of the Lister Institute, gave evidence upon the same topic, he got on very bravely until Dr. Wilson took him in hand, and then he candidly owned that “the chief preventive against tetanus is to ensure that wounds are washed clean and kept clean as possible.” Thus he gave the whole show away, and Mr. Paget and his filthy anti-tetanus quackery are left stranded.

I am, Sir, your obedient servant,

WALTER R. HADWEN, M.D., J.P.,  
President of the British Union for the Abolition  
of Vivisection.

Gloucester, December 26th, 1910.

*From the "CHELTENHAM EXAMINER."*

### **Research Defence Society.**

Sir,—In reply to your correspondent's letter of this week, let me say (1) Malta fever is often fatal. (2) His accounts of Bruce's work, and of the cases of accidental infection in English laboratories, and of the work of the Commission, are not in accordance with truth. (3) The symptoms of Malta fever are not like sea-sickness; and the germs of Malta fever are not like fat globules. (4) My figures were announced in the House of Commons, last June, by the Under-Secretary of State for the Colonies. (5) The civil population of Malta suffers less than our men, who come new to the place; still, it suffers. (6) Your correspondent's statements, that Malta fever is now called Simple Continued Fever, and that authorities are the laughing-stock of the island, and that the goats on the quarantine-station showed no signs of infection, are not in accordance with truth. (7) Of course, a man may drink goats' milk in Malta, yet escape the fever: not every goat yields infected milk. But will your correspondent consent to have a dose of the germs, in pure culture, put under his skin? Nothing with it; just the germs by themselves.

Let me this week take another instance of the value and the necessity of experiments on animals. I take epidemic meningitis: it is also called cerebro-spinal meningitis, or "spotted fever." Death from this disease is one of the worst ways in which a man can die. The patient suffers agonising headache, vomiting, and other miseries, till death comes as a merciful release; and, among those who recover, many are left blind, partly paralysed, or weak-minded. Dr. Robb, who had charge of the Belfast fever hospitals during an epidemic, calls it "the most terrible in its manifestations, and the most disastrous in its death-rate, of all the epidemic diseases met with in English-speaking countries."

By experiments on animals, Dr. Flexner and Dr. Jobling, of the Rockefeller Institute, proved that the disease is due to a particular kind of germs. They obtained these germs all by themselves, in pure culture; and reproduced the disease in monkeys by injecting under the skin a minute quantity of this pure culture. Your readers will be glad to hear that the disease in monkeys is less violent and painful than it is in man. By these and other experiments, Flexner and Jobling were able to prepare a serum-treatment for cases of the disease in man. This treatment was first used in the Spring of 1907. I beg your readers to study carefully the following tables:—

#### *Results before the use of the Serum.*

Of 4,000 cases in New York, in 1905, 75 per cent. died.

Baller reports from Greater New York, 2,113 cases, with 1,636 deaths, giving a mortality of 77.4 per cent.

Chalmers reports from Glasgow (1907), 998 cases, with 683 deaths, giving 68.4 per cent. mortality.

Baillie reports in Belfast (1907), 623 cases, with 493 deaths, giving 79.2 per cent. mortality.

Ker reports a mortality of 78 per cent. in the Edinburgh epidemic.

Robertson reports from Leith (1907), 62 cases, with 74.4 per cent. mortality.

Turner reports from the Transvaal, 200 cases, with 74 per cent. mortality.

In Ruchill Hospital, Glasgow, and Edinburgh Fever Hospital, and Belfast Fever Hospital, and the Boston Children's Hospital, the mortality ranged from 69 to 80 per cent.

*Results from the Serum Treatment.*

	Cases.	Deaths	Per cent.
City Hospital, Cincinnati ... ..	45	14	31.1
Dr. Dunn, Boston ... ..	40	9	22.5
John Hopkins' Hospital, Boston ...	22	4	18.1
Rhode Island Hospital ... ..	17	6	35.2
Lalleside Hospital, Cleveland ...	29	11	37.7
Edinburgh Fever Hospital ... ..	33	13	42.3
Mount Sinai Hospital (Children) ...	15	2	13.3
Municipal Hospital, Philadelphia ...	21	9	42.7
Belfast Fever Hospitals ... ..	98	29	29.6

Similar results have been obtained, with similar treatment, in France and Germany.

In one of his earlier papers, based on a large number of reports, Dr. Flexner stated that he excluded certain cases, practically hopeless, where the serum treatment did not have a fair chance. Excluding these cases, the mortality was 25 per cent. Including them, it was 33 per cent.

"From these figures," says Dr. Robb, "it will be seen that the death-rate, in the cases not treated with the serum, averaged some 75 per cent. This has been reduced, in cases treated with the serum, to less than half, and in many instances much below that figure. My own experience has been that of 275 cases under my own care in hospital, before the use of the serum was commenced, 72.3 per cent. died; while of the 98 cases treated with the serum 29.6 per cent. died. No selection of cases was made. . . . Great as this change in the death-rate has been, it is not more striking than the improvement in the course run by the cases; for, whereas it was common to have cases running on into weeks, and even months, such cases are no longer met with."

I shall be happy, Sir, to send any of your readers, who will send me seven stamps, two pamphlets published by the Research Defence Society, one by Dr. Bruce, on Malta Fever, and the other by Dr. Robb, on Epidemic Meningitis.—I remain, your obedient servant,

STEPHEN PAGET,

Hon. Sec. Research Defence Society.

21 Ladbroke Square, London, W.

December 24th, 1910.

From the "*CHELTENHAM EXAMINER*," Jan. 5th, 1911.

## Dr. Hadwen's reply to Mr. Paget.

### SPOTTED FEVER.

Sir,—Mr. Paget's cryptic comments upon my exposure of his claims concerning Malta fever are only what might be expected from such a source. It is his usual plan, namely:—pick out two or three side issues, flatly deny them, as if they represented the whole case, leave all the rest and incontinently hurry away with: "Let me take this week another instance of the value and necessity of experiments on animals." He then proceeds to give further lengthy extracts from his comical book, "*Experiments on Animals*," which he is furnishing to your readers in weekly parts.

It is weary work answering a correspondent who simply resorts to denial and evasions and provides no scientific evidence in support or defence of his assertions. Leaving his flat denials, I will just make a brief reply to his comments.

(1) He repeats: "Malta fever *is* often fatal."

I have proved from official figures that the mortality has never been more than two per cent. of the cases.

(2) He says: "The symptoms of Malta fever are not like sea sickness"

I did not say they were. I stated that no medical authority saw the men who were temporarily ill on board the *Joshua Nicholson*. There is no evidence as to their "symptoms." They were well when they left Malta and well when they landed. And it is just as likely as not that they simply had an attack of sea sickness. The only evidence vouchsafed is a very fallacious laboratory examination of their blood after they had recovered. The same result is frequently obtained in healthy people.

(3) He says: "The germs of Malta fever are not like fat globules."

I simply retort: "They are," and the chief bacteriologist of Malta admitted to me it was very difficult to tell the difference. Nobody expects Mr. Paget to know anything of the subject.

(4) He says: "My figures were announced in the House of Commons last June."

This is very amusing, as if Mr. Paget had ever framed any statistics in his life! The figures he mentions were the official statistics of the War Office, which I had already published in my *Contemporary* articles months before, and on the occasion Mr. Paget refers to they were given by Col. Seely in reply to a question put to him by a Parliamentary member of my Committee. As they were a pure evasion, another question was subsequently put to him which compelled him to admit the truth of my contention, that practically all the Malta fever had disappeared from among the troops in Malta months before the goats'



milk was stopped. And he also had to admit that this cessation of the fever was coterminous with the removal of the soldiers from St. Elmo Barracks, described by one of the native doctors in a Maltese journal as "a veritable ditch." But all this Mr. Paget is careful to avoid. He relies on the evasion and subterfuge, "My figures were announced in the House of Commons!"

(5) He says "The civil population suffers less than our men who come new to the place."

They do—some thirty times less! He hints, without saying it outright, that it is because the natives are acclimatised. His caution is doubtless due to the fact that I have already exposed this fallacy by showing:—

1st—That Malta fever is alleged by his party to be due to goats' milk and not to "place."

2nd—That as the sailors have only suffered half as much as the soldiers there must be some other cause than goats' milk.

3rd—If Malta fever be due to goats' milk, then it should produce the fever in the natives at some time early in their lives in order to render them immune, and there should be thousands of cases annually, but there are no statistics in existence to support such a theory. There have never been more than 500 cases annually out of a population of over 200,000.

4th—The statistics are the other way; the cases in civil life are usually about middle age and cases of 70 and 80 years are recorded in persons who had drunk goats' milk all their lives.

5th—Dr. Agius, of Malta, records that he contracted Malta fever through inhaling sewer gas although he had never tasted goats' milk in his life, and he was nursed back to health again by drinking goats' milk, which he has consumed ever since.

6th—Malta fever cannot confer an immunizing effect upon the civil population as Mr. Paget suggests, as repetitions of the disease when once contracted are frequent.

7th—I found by enquiries that our soldiers and sailors rarely drank milk at Malta except with tea, but the Maltese themselves are great milk drinkers, and this, combined with the fact that they lived under healthier sanitary conditions than the garrison, accounted for their greater freedom from the disease.

(6) Mr. Paget asks: "Will your correspondent consent to have a dose of the germs, in pure culture, put under his skin?"

I want to know why I should make such a fool of myself? I leave all that sort of thing to innocent and superstitious people like himself.

My answer to Mr. Paget's fresh quotations—nearly all from America—about "spotted fever" is very simple. We had a run on "spotted fever" in this country lately—a complaint without either "spots" or "fever" (I have seen some of them), and if all these have been counted into the Edinburgh and Belfast statistics whilst the scare was on, the decrease is easily understood; it is now admitted that a number of cases of ordinary infantile paralysis have been doing duty for "spotted fever," and thus all the statistics are vitiated.

But Flexner's statistics, upon which Mr. Paget chiefly relies and which he has circulated broadcast are simply fraudulent. Mr. Paget



tries to anticipate my reply to him (for he knows what is coming) by giving a garbled account of Dr. Flexner's excuses about them. He says Dr. Flexner "excluded certain cases, practically hopeless, where the serum treatment did not have a fair chance. Excluding these cases, the mortality was 25 per cent. Including them it was 33 per cent."

It seems quite impossible for Mr. Paget to give the whole truth about anything. I never had to deal with such an elusive controversialist. Allow me to quote Dr. Flexner's own words in full :—" In making up the various statistical tabulations no selection of cases has been made further than to exclude, first, those cases which survived the first dose of the serum less than twenty-four hours; a second elimination embracing a smaller number of cases, includes the rapidly fulminant cases; a third small elimination includes several cases of secondary and mixed infection," etc.

This method of "making statistics" is one of the most delightful specimens of impudent cooking I have ever come across. It is quite clear that the rejected were not "practically hopeless" as Mr. Paget says, or the serum would not have been given. But one thing stands out very clearly—the serum absolutely fails where it is most needed, and the fact of certain cases "surviving the first dose less than twenty-four hours" gives one "furiously to think"! No wonder Dr. Flexner has now given up making his precious serum! He is evidently anxious for his credit.

But outside his institute, in his own city, according to the returns of the New York Health Department, the disease claims just as many victims as ever.

I am, Sir, Your obedient servant,

WALTER R. HADWEN, M.D., J.P.,

President of the British Union for the Abolition  
of Vivisection.

Gloucester, Jan. 2nd, 1911.

*From the "CHELTENHAM EXAMINER."*

### **Research Defence Society.**

Sir,—Let me this week say something about diphtheria antitoxin. It was discovered by means of experiments on rabbits; and it has been in daily use all over the world for fifteen years. The lives which have been saved must be reckoned, literally, by the hundred thousand. We owe this blessing mainly to the work of Behring and Kitasato. They started with the fact that the germs of diseases, wherever they are, brew certain poisons, each according to their nature. They brew these poisons wherever they are, in the blood of a man, or in the blood of a mouse, or in a test tube. These poisons are called toxins, from the Greek word for poison. The germs of diphtheria brew their own special toxin, or poison. It is this toxin which produces the high temperature, drowsiness, vomiting, and swollen glands, of a bad case of diphtheria. The toxin is not the germs themselves; it is the chemical poison which the germs brew. As poppies brew opium, and cinchona trees brew quinine, so diphtheria germs brew toxin.

Behring and Kitasato found a way to protect rabbits, so that they could not take diphtheria, not even if the germs were rubbed on the back of their throats. Rabbits, in their natural state, can easily be infected with diphtheria; but these protected rabbits simply could not take the disease. They had been protected by the following method:—A very small dose of the toxin was given to them, not enough to kill them. The next day they could take a rather larger dose; the next day a still larger dose; till, at last, they could take, without turning a hair, a dose of toxin large enough to kill an ordinary rabbit. The gradual action of the toxin produced a change in their blood, I mean a chemical change. There was some chemical change in their blood which withstood the disease.

Next, it was discovered that their blood acted as an antidote. If a little of the clear part of their blood was taken, and was mixed with some toxin, and this mixture was put under the skin of a fresh series of rabbits, these rabbits did not die. There was enough toxin to kill them; but the blood, which was mixed with the toxin, stopped the toxin from killing them. The blood of the protected rabbits saved the lives of the unprotected rabbits. It prevented the toxin from killing them. Their blood, therefore, is called antitoxin.

Next, it was discovered that this antitoxin could catch up the toxin, even if the toxin had forty-eight hours start. Thus, if the toxin were given on the Monday, and the antitoxin on the Tuesday, or even on the Wednesday, the rabbits would mostly be saved.

Next, it was found that horses, like rabbits, could be immunised; and thus could be used for the direct service of man. These horses receive small doses of toxin, till their blood contains antitoxin. They are then bled, under proper antiseptic and aseptic precautions, and

the clear part of the blood is put up in little bottles. A dose of this antitoxin is given through a hollow needle under the skin in cases of diphtheria.

I should like to say a word about the condition of these horses. To begin with, their general health is carefully tested before they are used. Then the immunising does not hurt them. They may be off their feed for a short time, or just poorly for a short time, but not more than that. Of course, I have seen them in a big establishment near London. They were one and all of them sleek, healthy, well-fed, idle, comfortable creatures. Any cab horse might well envy them. I saw the processes of immunising and of drawing off the blood. The horses treated each affair with the utmost indifference, and went back placidly to the stables.

Fifteen years of incessant use all over the world is good evidence. Go round the whole earth—London, Paris, Berlin, Rome, Vienna, St. Petersburg, India, China and Japan, Australia and New Zealand, San Francisco, Chicago, Boston, New York, Canada—wherever you go it is the same story, of the lives of children saved, whole legions of them.

Fifteen years ago it was possible to compare hospitals which used the antitoxin with hospitals which did not. It came into use in 1894. Two hospitals in Paris, in that year, may be compared. One used it, the other did not. The death-rate from diphtheria at the one was 24.5 per cent.; the death-rate at the other was 60 per cent. At Zurich the mortality was brought down, with the antitoxin, from 39.4 per cent. to 12 per cent. Among the French Army it was brought down from 11 per cent. to 6 per cent. In the vast collected figures for 266 German towns it was brought down, between 1894 and 1897, from 101 to 35 per 100,000 of the living. These are but a few of the early experiences with antitoxin.

If we take the Registrar-General's returns for England and Wales, we find, in spite of antitoxin, a terrible lot of deaths from diphtheria. But we must remember three things : (1) Thousands of cases which used to be called "croup" are now called diphtheria; (2) compulsory education, with its vast increase of big classes in crowded schools, helps to spread the disease; (3) there is but one way to judge the value of a drug. Imagine a thousand cases of diphtheria. Divide them into two groups of 500 each, treat the one group with antitoxin and the other without. Which of the groups would have the larger mortality? There is not the very faintest shadow of doubt as to the answer to the question.

It is sometimes said that the antitoxin is given all round in our big fever hospitals, even to children who have only a trivial sore throat. That is not true. It is also said that the antitoxin causes those occasional paralyses which occur after diphtheria. That is not true. It is also said that we cannot trust the figures given by the Metropolitan Asylums Board, because they include, as cases of diphtheria, children who have the germs in their upper air passages, without having the usual symptoms of the disease. But these cases have been excluded in the last report of the Board. Anyhow, there were only 275 of them; they make no difference one way or the other. It is also said that the mortality among children treated without antitoxin at our fever hospitals is less than the mortality among children treated with antitoxin. But, if you take the bad cases, you find that the mortality among the cases

treated with antitoxin was 140 deaths in 895 cases, which is a mortality of 15·6 per cent., whereas of 15 laryngeal cases which did not get the antitoxin, 14 died, which is a mortality of 90 per cent. These unhappy babies were dying on admission, and it was too late to save them. The children who got well without antitoxin were the children with trifling sore throats, which did not want the antitoxin, and did not get it.

Finally, let me draw the moral from the rabbits to the babies. The moral is, that the antitoxin must be given at once. It should be given on the first day of the disease. I do not say that no ill results among the many hundreds of thousands of cases, under wholly exceptional conditions, followed the use of antitoxin. But I do say, most emphatically, that it has saved legions of lives and has been one of the best blessings ever given to medicine.

By an odd chance I have at this moment received a letter from the Medical Health Officer at Toronto. Certain figures were quoted at a meeting of the present Royal Commission from Toronto. It was said that the antitoxin had failed in Toronto. The figures were quoted from a report six years old. I am glad to say that the Medical Officer of Health assures me that the results obtained from the use of antitoxin have been quite as gratifying in Toronto as elsewhere.

I remain, Sir, your obedient servant,

STEPHEN PAGET,

Hon. Secretary, Research Defence Society.

21 Ladbrooke Square, London. W.

January 3rd, 1911.



From the "*CHELTENHAM EXAMINER*," Jan. 12th. 1911.

## Dr. Hadwen's reply to Mr. Paget.

### DIPHTHERIA ANTITOXIN.

Sir,—I am afraid Mr. Paget is becoming exhausted. He has left my replies concerning typhoid fever and tetanus absolutely alone, contenting himself with "Let me say this week something about diphtheria antitoxin." He then proceeds with his stereotyped letter upon the subject, which we are all weary of seeing in print and answering. One quite longs for something original from his pen.

He begins, of course, by saying that "the lives which have been saved by antitoxin may be reckoned by the hundred thousand." He goes on to say a little further on that the numbers consist of "whole legions of them," and winds up at last by reiterating: "I do say, most emphatically, that it has saved legions of lives." But, unfortunately, he forgets to furnish evidence that even one single life has ever been saved by this extraordinary production! It is like the story of the boy and the cats on the tiles.

He has a good deal to say about "germs which brew poisons"; about injecting germs into rabbits and rubbing the backs of the throats of these poor little creatures with them; about antitoxin having a sort of neck to neck race with toxin along the course of the blood vessels and catching it up at the end of 48 hours; of pumping diphtheria germs into horses (although they never have diphtheria), and finally bleeding them and putting the serum of their blood into tubes which are sold at a thousand per cent. profit under the name of antitoxin; of the happy condition of the horses which are thus poisoned and bled in the El Dorado of a chemical laboratory; but not a solitary word to show how the "legions" and the "hundreds of thousands of saved lives" are calculated!

In fact, his own evidence goes to prove the opposite, for with that strange penchant Mr. Paget possesses for always giving his case away, he says:—"If we take the Registrar-General's deaths for England and Wales we find, in spite of antitoxin, a terrible lot of deaths from diphtheria"! Consequently, he is obliged to leave England. He tells your readers to "go round the whole earth—London, Paris, Berlin, Rome, Vienna, St. Petersburg, India, China and Japan, Australia and New Zealand, San Francisco, Chicago, Boston, New York and Canada," in order to find the "legions of saved lives" which he talks about!

He says "There is but one way to judge the value of a drug." It is as follows:—"Imagine a thousand cases of diphtheria. Divide them into two groups of 500 each, treat the one group with antitoxin and the other without. Which of the groups would have the largest mortality"? And then he stops, with the "imagination" on tenter-hooks, without affording the slightest clue as to how his readers are to solve the riddle and without providing a shade of evidence to assist in divining the answer. It is quite a new idea as to how to "judge the value of a drug." By a process of "Imagination"!

Unfortunately for Mr. Paget the Registrar-General's Returns *do* show "a terrible lot of deaths from diphtheria in spite of antitoxin." They prove that during 15 years since antitoxin has been introduced into this country, the death-rate from diphtheria to the living population has increased 25 per cent. above the 15 years antecedent to its use. Hence it is not surprising that Mr. Paget has to roam all over the

world to find his imaginary "legions" in lands where Registrar-General's statistics are not so easily available as here.

But, says Mr. Paget, one reason for these deaths is that "Thousands of cases which used to be called 'croup' are now called 'diphtheria'!" Mr. Paget always deals in large numbers. Well, let the Registrar-General speak for himself. Here are the figures for croup and diphtheria together for the last 40 years in ten-year periods: Average annual death-rate per million of the population:—

1861-70.	1871-80.	1881-90.	1891-1900.
390.	261.	286.	314.

It would be interesting to know how Mr. Paget explains the fact that the death-rate from diphtheria and croup combined was only 261 per million in 1871-80, and yet in 1881-1900, after six years of antitoxin treatment, it has risen to 314? If, as he says, all the cases in the latter group are genuine diphtheria, whereas those of the former were diluted with croup, so much the worse for his argument. Instead of his "legions of lives saved," the actual facts point just the other way.

His next excuse for the Registrar-General's figures being so much against him is unfortunately no better. He says:—"Compulsory education, with its vast increase of big classes in crowded schools, helps to spread the disease." I quite agree with him, but that does not help his case. We are not dealing with the *increase of cases*, but with the *saving of life*. If the cases increase, so much the better is the opportunity afforded for antitoxin to save the patients from dying, but if, as he says, "in spite of antitoxin, the Registrar-General's returns show a terrible lot of deaths," I am afraid Mr. Paget is in a worse pickle than ever.

He goes on to talk of the Metropolitan Asylums Board figures. Even here "the legions" are not forthcoming. They publish two tables, the very thing Mr. Paget asks for, namely, those treated with antitoxin and those without, and in the latter table the decline in the death-rate is double as much as that in the former. So Mr. Paget begins making excuses again; he says those treated "without antitoxin" were those that had diphtheria, but got well without it! I am afraid that that does not help him much. But alas, that table also contains all the very worst cases, a fact which vitiates the other table he relies upon and makes *all* the statistics ridiculous. It also shows that antitoxin is powerless where it is most needed. We are still left without evidence of the "legions."

"Finally," says Mr. Paget, "let me draw the moral from rabbits to babies." A most interesting sequence! And he adds: "I do not say that no ill results among the many hundreds of thousands of cases, have ever, under wholly exceptional conditions, followed the use of antitoxin." That is a serious admission. Alas, there have been a great many of such unfortunate cases, and any mother's child may any day be one of the "exceptions."

Mr. Paget, therefore, has left antitoxin in a very parlous condition. His "legions of saved lives" remain where he himself has placed them—in the "imagination," but the possible deaths, as the results of this filthy, useless, and dangerous quackery, are frankly admitted by his own pen.—I am, Sir, Your obedient Servant,

WALTER R. HADWEN, M.D., J.P.,

President of the British Union for the Abolition  
Gloucester, January 9th, 1911. of Vivisection.



*From the "CHELTENHAM EXAMINER."*

### **Research Defence Society.**

Sir,—Your readers can easily test the statements in the letter which you published last week about Malta fever. They can show the letter to any medical man in Cheltenham or elsewhere, or to any medical student, and ask him what he thinks of it. There is no need for me to answer charges of evasion, subterfuge, superstition, etc., nor to defend Dr. Flexner against charges of fraud and impudence. I prefer to go on with my proper business of making known the facts about experiments on animals. I have already written about plague, Malta fever, typhoid, tetanus, epidemic meningitis, and diphtheria. I should like to say something this week about tubercle.

By the word tubercle, I mean not only consumption, but also that vast group of other disease which are due to the tubercle bacillus, hip-disease, spinal caries, tuberculous glands, lupus, etc. But we may take consumption as the type of all the tubercular diseases.

Laennec, the inventor of the stethoscope, said, nearly a hundred years ago, that consumption was a definite and distinct disease, apart from other inflammations of the lung. Before his time, this distinction had not been generally recognised. Yet we find, and may well be surprised to find, that the disease was recognised as infectious, ages before Laennec. Thus, five hundred years ago, Frascatorius had said that consumption might arise "from the passing of the corrupt and noisome humours of the patient into the lungs of a healthy man." And, in France, in the seventeenth century, consumption was admitted to be infectious, and some sort of isolation was practised to keep it from spreading. But, since the true nature of the disease had not been proved, at that time, by experiments on animals, people gradually lost sight of the fact that it may be communicated by infection.

In 1843, Klencke, by experiments on animals, showed that tubercle could be conveyed, by inoculation, from one animal to another. In 1865, Villemin proved, by experiments on animals, that the disease is due to a specific living agent. By the inoculation of rabbits, he proved that tubercle could be multiplied indefinitely in animals, from one to another; he also demonstrated the identity of consumption with other forms of tuberculosis. Three years later, Chauveau produced the disease in animals, not by inoculation, but by mixing tuberculous matter with their food. Then, as the study of the disease advanced, there came a long period of uncertainty, because animals differ so widely in their susceptibility to tubercle. But, by 1880, this period of uncertainty was at an end; and Cohnheim was able to say: "Everything is tuberculous which can produce tuberculous disease, by inoculation, in animals susceptible to that disease: and nothing is tuberculous which cannot do that." Next year 1881, came the welcome news that Koch had discovered the tubercle bacillus, the germ itself, the actual cause of the disease.

To help us to understand the world-wide importance of this great discovery, we could not have a better guide than Koch's own words, published in March, 1882—"Henceforth, in our warfare against this dreadful scourge of our race, we have to reckon, not with a nameless something, but with a definite germ invading the body: the conditions of its life are for the most part already known, and can be further studied. . . . Before all things, we must shut off the sources of infection, so far as it is in the power of man to do this."

Practically, as we look back over a hundred years of the study of consumption, the names which stand out are Villemin, Pasteur, and Koch. Villemin and Pasteur set Koch to work. Thanks to him, the whole world learned at last the true nature of consumption, the actual, material, living cause of the tubercular diseases. Men understood, at last, what it was that they were fighting. Half a century ago, neither the general public nor the doctors ever thought of a national crusade against consumption, or imagined that the disease would be fought on national lines. It is absolutely certain that the present more hopeful and more rational views of consumption, and the present general use of the open air treatment are both of them founded and built, not on practice alone, but also on science. They are the result, not only of bedside observation, but also of experiments made on rabbits and guinea pigs. Before 1881, no doctor had ever attempted to diagnose a case of consumption by examining the sputa under a microscope. We owe it to experiments on animals, and to them alone, that every medical man is better able to-day to detect consumption in its earlier stages, when there is most hope that the patient's life will be saved.

Again, a great part of the usual precautions now observed in the nursing of consumptive patients, comes direct out of Koch's work. The same is true of the enforcement of rules against spitting in public places. And the same is true of the present plans to make consumption a notifiable disease. All these improvements in the service of the national health are due to experiments on animals.

Again, the present use of the "new tuberculin" is giving good results, and has helped to save or prolong many lives.

Again, experiments on animals gave us the present admirable method of detecting the disease even in its earliest stages, in cattle. If a cow has a little patch of tubercle in its lung, or in its udder, and if it be tested with a dose of tuberculin put under the skin, it re-acts: that is to say, the cow is feverish and off its feed for a few days. If the cow be free from all disease of a tuberculous nature, it does not re-act to the tuberculin. This test may fail if it be used on cattle knocking about on board cattle-boats, or just landed at the docks: and one can hardly imagine any test which might not fail under such conditions. But, as a dairy test, or as a test for the breeding and exporting of cattle, this use of tuberculin is everywhere approved and employed, as a matter of routine, in all the more civilised countries of the world. Indeed, they say in our own country that cunning dairy-men will even fake their tuberculous cows, giving them a dose of tuberculin a day or two before the Inspector is likely to come, so that the cows may get their re-action over before he arrives, and may give no further re-action when he tests them. Again, the London milk supply is tested by the help of inoculation of guinea pigs with any sample suspected of containing the germs of tubercle. Samples of

milk, from many dairies, are sent to the Lister Institute, by order of the London County Council, for this purpose. I have seen these inoculated guinea pigs at the Lister Institute, and not one of them was in any pain. A tuberculous gland, or a tuberculous nodule, gives no pain to us, and cannot give pain to a guinea pig. By this very simple test we are guarded, and our children are guarded, against the spread of tubercle in the milk supply. This is only one of the many instances where the real "vivisectors" are the British public, the Government, and the Public Councils.

I do not wish, in any way, to evade the fact that the first use of tuberculin, the "old tuberculin," failed. That failure happened more than twenty years ago, and our men of science have advanced far beyond the point where things stood in 1890. Nor do I wish in any way to evade the fact that the general mortality from tuberculosis had been going down for many years, before the cause of the disease was discovered. That is not good enough security for the world's health. Nor do I wish in any way to evade the fact that the exact amount of danger from the drinking of unboiled milk seemed much less to the mind of Koch than it seemed to the minds of other men of no less authority. Koch did not think that this danger was great enough to warrant severe and burdensome restriction of the milk trade; other experts thought it was.

Look which way we will to-day, either at the diagnosis of consumption, or at its prevention, or at the present general advance of the whole country to a crusade against this national evil, we find everywhere the results of Koch's discovery. Among all the many gifts which have come, by the help of experiments on animals, not only to men, women, and children, but also to animals, surely not the least is the discovery of the true nature of tuberculosis.

I remain, Sir, your obedient servant,

STEPHEN PAGET,

Hon. Sec. Research Defence Society.

21 Ladbroke Square, London, W.,

Jan. 7th, 1911.

*From the "CHELTENHAM EXAMINER," Jan. 19th, 1911.*

## **Dr. Hadwen's reply to Mr. Paget.**

### **CONSUMPTION.**

Sir,—If Mr. Paget is satisfied by ignoring the definite proofs I have provided of gross misrepresentations upon his part, of evasions of serious charges against him and his methods, of ignorance of proven facts, and of general distortion of historical evidence, I have no reason to complain and the public will draw their own conclusions. As Honorary Secretary of what he has called the "Research Defence Society," one would have thought his "proper business" is to defend his position and his claim, but he says his "proper business is to make known the facts about experiments on animals," and to take no notice of any exposures of his "facts." He refers your readers instead to "any medical man in Cheltenham," the majority of whom would know no more of the subject than does Mr. Paget himself, and who, if appealed to, would in all probability merely parrot the very same absurdities that Mr. Paget has himself collected, or candidly confess that they know nothing of the subject. I will give Mr. Paget credit for consistency of method: he stands in a fortress for a few moments and makes an oracular demonstration. As soon as his fortress is riddled with bullets and it becomes untenable, he moves on to another one, and says, "I should like to say something this week about so and so," and when this fortress becomes untenable, he moves on again! This is "instructing the public"! I should call it "amusing" them!

However, "this week" it is "tuberculosis" or consumption, and its treatment. As usual, Mr. Paget winds up by giving his case away. He admits that the "old tuberculin" which worked such marvellous miracles and brought such honours to its discoverer has "failed." He says the "new tuberculin" has "helped to cure or prolong many lives," but as he does not know how long the poor creatures who have been experimented upon with it in hospitals might have lived without it, this is pure guess work, and as no medical man with a reputation to lose will pin his faith to the "new" abomination, one can well understand Mr. Paget's cautious comment.

He further admits that "the general mortality from tuberculosis had been going down for many years before the cause of the disease was discovered," which leads one to presume that the decline would most probably have continued even if the so-called "cause" had never been "discovered" at all.

He also admits that the great "discoverer" came to the conclusion in his riper years that anyone might drink unboiled milk without any fear of danger—and he ought to know most about it—as the germs it might contain could not do any harm.

After all these naive admissions by Mr. Paget there is very little else for me to say; he has simply "cooked his own goose" and proved without any assistance from me that the germ theory in relation to tuberculosis is as visionary as it is in regard to every other disease we have discussed in these columns.

However, I think I can help to demolish his own case even more effectively than he has done already. I need not refer to all the disgusting and ridiculous experiments he quotes at length which were performed upon living animals—mainly guinea pigs and rabbits—in



order to elaborate the idea with which Mr. Paget's mind is obsessed. I will settle all that in a single sentence by quoting no less an authority than the "discoverer" himself, Professor Koch, of Berlin, who says on page 8 of his "Cure of Consumption":—"Here again is a fresh and conclusive proof of that most important rule for all experimentalists, that an experiment on an animal gives no certain indication of the result of the same experiment upon a human being."

Dr. Muthu, of Mendip Hills Sanatorium, has recently declared in his elaborate work on Tuberculosis that he found the tubercle bacillus absent in 30 per cent. of his cases of tuberculosis. How then can it be the cause of the disease? This germ is practically never found in the initial stages of the disease, when, if at any time, it ought to be present. And, moreover, other bacilli, such as the Timothy grass bacillus and the Smegma bacillus, which are confessed to be quite harmless, and are found in myriads about every dairy, cannot be distinguished by any test from the so-called genuine article. The whole idea is an absurdity.

What has been proved is this: That vitiated air, added to hereditary predisposition, or contracted lung trouble from cold, etc., will produce the disease, and that fresh air in many cases—that is, a complete change of environment—will cure it. This is scientific common sense, and requires no experiments upon living animals for its elucidation, nor have such experiments assisted it in the least. On the contrary, they have retarded the beneficent method of nature, by leading the medical profession on to false lines, and by being the direct cause of premature deaths.

Mr. Paget's final effort to show that the tuberculin which was found useless for human beings had been found valuable for testing cattle, whilst at the same time inferentially admitting that the germ in both cases is the same, is too comical for words.

But Mr. Stockman, Veterinary Officer to the Local Government Board, showed in his evidence before the Royal Commission recently that this theory is full of fallacies and the test affords the public no "guarantee" whatever. It was found fallacious when applied to cattle reared on foreign ranches when landed at Birkenhead. It could not be applied with certain results to the cattle in a show yard or in the market. They might react and leave you in doubt even though tuberculous. In cases where the disease is tolerably advanced they do not react to tuberculin at all.

The Department of Agriculture of the University of Aberdeen has published a report showing that out of 240 cattle tested with tuberculin, 24 of those which gave no reaction and were consequently pronounced healthy, were found *post mortem* to be tuberculous. And Professor Brouere, of the Veterinary Department of the Board of Agriculture says it has spread Tubercle Bacilli where the body was not previously infected.

Therefore when Mr. Paget declares that his "proper business is to make known the facts," there is no other conclusion to draw but that Mr. Paget has never yet learned his business properly.

I am, Sir, Your obedient servant,

WALTER R. HADWEN, M.D., J.P.,

President of the British Union for the Abolition  
of Vivisection.

Gloucester, Jan. 16th, 1911.



*From the "CHELTENHAM EXAMINER."*

### **Research Defence Society.**

Sir,—With regard to diphtheria antitoxin, if any of your readers would care to have a copy of the actual tables and charts published by the Registrar-General, and by the Metropolitan Asylums Board in its latest report, I shall be happy to send these facts, in return for a post card. Your readers will then see that I have given a true account of the case. Other statements in your correspondent's letter may easily be tested, by the test which I have already mentioned. The letter can be submitted to any medical man in Cheltenham or elsewhere, or to any Medical Officer of Health, or to any medical student, or to any Hospital Sister. That is an easy and fair test of your correspondent's letters.

My instances of the value and the necessity of experiments on animals are not yet exhausted: and, when your readers are tired of instances in pathology, they shall have instances in physiology. Meanwhile, I take, for another instance in pathology, Pasteur's discovery of the preventive treatment against hydrophobia.

In our own country, hydrophobia (rabies) has been stamped out, Heaven be praised, by the Muzzling Act, and by quarantine of dogs. We live on an island: it was therefore possible to stamp out rabies by these methods. On the Continent, and in India, different conditions prevail. It would not be possible to muzzle all the stray dogs in Constantinople. It would be equally impossible to muzzle all the wolves in Russia. Happily, it was possible to muzzle all the dogs in England.

Two facts, here, are of great importance. (1) It was Pasteur himself who proved, by experiments on animals, that rabies is not a disease which "comes of itself"; and it was this proof which made our Government sure that a thoroughly effective Muzzling Act would really put an end to rabies in this country. It was Pasteur himself who advised the Committee, appointed by our Government, to be content with a strong Muzzling Act. This Committee was appointed by our Local Government Board, about 1887, to enquire into Pasteur's preventive treatment. The Committee went to Paris, and Pasteur said to them, "You do not require my method in England at all. I have proved that rabies is an infectious disease. All that you have to do is to enforce a short quarantine covering the incubation period, and to muzzle all your dogs at the present moment: and, in a few years, you will be free." (2) Pasteur's advice to our Local Government was zealously backed by "the vivisectors." It was the "anti-vivisectors" who have to bear the disgrace of stubborn and wild opposition to the Muzzling Order. They denounced, in vehement language, Mr. Walter Long and his Act. It does not matter now what they said. But the fact remains, that the worst enemies of the preventive treatment against rabies were also the worst enemies of the Muzzling Act.

Let me give some account of the way in which Pasteur was led to discover the preventive treatment. By his earlier work, on fowl cholera and on anthrax, he had learned the general principles of immunity. He could standardise this or that disease. He could store, in test tubes, this or that disease in every shade of strength, from non-virulence up to full virulence. He could have a "fixed virus": that is to say, he could have, in a test tube, a certain substance, which would produce, in a known time, a known result, neither more nor less. He could exactly graduate his doses, beginning at zero, and advancing day by day, point by point, to a dose of full strength. By this gradual method he could immunise dogs or rabbits against rabies, so that it was impossible to give them the disease. He could also, after dogs or rabbits had been infected with the disease, prevent it from flaring up in them.

If a man is bitten by a rabid animal, and is carrying the poison about with him in the scar, there is a period of time before the poison gets into his central nervous system, his brain, and his spinal cord. This period may be long, or it may be short: anyhow, there always is a period, which is called the period of incubation, before the poison gets too far into the system.

By experiments on dogs, Pasteur found that the first signs of rabies occurred fifteen days after the poison had finally got hold of the brain and spinal cord. Once it has got so far as that, it is too late for the preventive treatment to have a proper chance. Therefore, if a patient at the Pasteur Institute dies within fifteen days of the completion of the treatment, the Institute very properly calls attention to the fact. The anti-vivisectionists are not so scrupulous as that; they add these cases to their lists of Pasteur's "failures."

The following method is in general use:—Rabbits, not dogs, are used: and your readers will be glad to know that, in rabbits, the disease assumes a paralytic and almost painless form, wholly different from the misery which it inflicts on dogs, let alone the bodily and mental agony which it inflicts on man. The rabbits are inoculated with a measured dose of the disease, of standard strength. They develop the standard disease: and they die in the standard time. When they are dead, their spinal cords, which contain the disease, are removed, under antiseptic precautions, and are dried in bottles. As the cords dry, they gradually lose strength, point by point, day by day. A cord that has been dried for a fortnight has lost all strength. One that has been dried for thirteen days has a very faint shade of strength: one dried for twelve days is a shade stronger, and so on. The patient begins with a minute dose of a cord fourteen days old, rubbed up in a little water, and put under the skin through a hollow needle. Next day, he can take, without harm, a dose of cord thirteen days old: next day, a dose of cord twelve days old, and so on. When he has attained a dose of a cord only four or three days old, the treatment is complete.

Now, from the study of more than 3,000 cases, it appears that the risk of death, if a man be bitten by an animal supposed, on reasonable grounds, to be mad, is 16 per cent., if the patient be not treated at a Pasteur Institute. Of these 3,000 persons, 500 died in all the dreadful agony of rabies.

With the Pasteur treatment, the mortality is not 16 in 100. It is about one in 250.

I do not say that no failures, and no mischances, have ever occurred in the twenty-six years during which the treatment has been in use, not only in Paris, but in diverse parts of the civilised world. But I do say, most emphatically, that thousands and thousands of lives have been saved, by the preventive treatment, from one of the worst deaths that a man can suffer.

Every patient at the Pasteur Institutes, in Paris or elsewhere, is divided into three classes, (1) those cases where the biting animal was said to have been mad; (2) those cases where the biting animal was demonstrated to have been mad, because its body was examined after death, by a veterinary surgeon; (3) those cases where the biting animal was proved to have been mad. Either it bit other animals, and they went mad: or, a particle of its spinal cord, after death, was put into another animal, and that animal went mad.

We shall all be agreed that the patients in this third class were undoubtedly bitten by animals that were mad. I have before me the latest report from the Pasteur Institute in Paris. The number of patients treated, during 1909, was 467. Of these 467 persons, two died, one of whom died within fifteen days of the completion of the treatment. The number of cases in the first class was 173; none of whom died. In the second class, 220 cases, two of whom died. In the third class, 84 cases; none of them died. The two patients who died were Henry Watson and George Seaman, the two English soldiers who were bitten at Gibraltar on September 12th, 1909.

At the great Pasteur Institute in Tunis, during 1909, the number of persons treated was 354. But three of them were members of the Institute who had not been bitten, but underwent the treatment because of some other reason: two patients left off the treatment for no particular reason: and nine left it off because the animal was found after all not to have been mad. Thus we were left with 340 cases. Of these 340 cases, 180 were in the first class; 125 in the second class; and 26 in the third class. Not one of the 340 cases died.

The total statistics for 21 years at the great Pasteur Institute in Charkow have just been published. Between 1881 and 1909, which is 21 years, 24,051 cases were treated: 19,581 in the first class, 2,393 in the second class, and 2,077 in the third class. Including the cases which died within 15 days of the completion of the treatment, the mortality was 1.1 per cent. Excluding these cases, it was 0.67 per cent. —

I remain, Sir, your obedient servant,

STEPHEN PAGET,

Hon. Secretary, Research Defence Society.

21 Ladbroke Square, London, W.

January 16th, 1911.

From the "*CHELTENHAM EXAMINER*," Jan. 26th, 1911.

## Dr. Hadwen's reply to Mr. Paget.

### HYDROPHOBIA.

Sir,—As Mr. Paget's only rejoinder to my reply on Antitoxin is a further pathetic appeal to the medical practitioners of Cheltenham to assist him and an offer to supply pamphlets which I have already exposed in your columns, I am saved the necessity of saying anything further about the subject. Your correspondent's pamphlets are duly appearing week by week; they are now nearly exhausted and that will end Mr. Paget, so your readers have no need to be alarmed at his threat to commence a fresh series of reproductions when the present series is concluded. I know his limits!

According to his usual plan of replying to one subject by dealing with another, we are "this week" treated to Hydrophobia.

It is rather amusing to be told that Pasteur advised muzzling as quite good enough for England, whilst he considered his virus was necessary for France. Pasteur, who in all these matters was a pure charlatan, knew very well that if his virus were to be used in this country the fraud of it would speedily be discovered. As it is, no medical man of reputation in England has ever committed himself to Pasteur's treatment of hydrophobia.

I need not touch upon the long dissertation Mr. Paget gives upon the filthy and horrible method of preparation, which occupies half a column. What is more to the point is this:—That I have a collection of some 3,000 cases of human beings treated for hydrophobia at the various Pasteur Institutes on the Continent and in India, who have died from the disease at varying periods after treatment. A fresh series of fatal cases is now being published in the *Abolitionist*. In many of these cases there was no evidence that the incriminated dog was mad at all—thus emphasising the truth of the charge made by a great French doctor, "Pasteur does not cure hydrophobia, he *gives* it."

Mr. Paget claims that in a certain number of cases the dog *was* proved to be mad. How? By inoculating the brains of rabbits with a portion of the brain of the alleged mad dog. But the fallacy is this. As Mr. Paget confesses, the process does not make the rabbit mad at all, but only paralyses its hind quarters, and precisely the same result was produced by Vulpian by injecting the brain of a rabbit with healthy saliva. Hence this cruel experiment is no proof whatever and the statistics founded upon it are therefore fallacious.

He points out the low death rate among Pasteurian patients. The reason is not far to seek. So great a scare has been created of late years, that no lady's hand can be grazed by the teeth of her lap dog without her being ordered to Paris to be inoculated for hydrophobia, and in fact things have reached such a ridiculous pitch, that Lord and



Lady Minto recently submitted themselves to inoculation because *somebody else* had been bitten, and they wished to be "on the safe side." One morning, at the Pasteur Institute in Paris, I saw about 100 persons of both sexes and all ages inoculated; I made careful enquires among them and I found that there was not one out of the whole number who had any positive evidence as to having been bitten by a mad dog. In fact, it was a question which did not appear to occur to them. No wonder the death rate is so low. The statistics are a complete fraud.

Upon the very face of them they are fraudulent, inasmuch as for 30 years before inoculation was invented there was not more than an average of 30 deaths in France from hydrophobia out of 40,000,000 of people. For 30 years after inoculation the average number of deaths with a less population rose to 100. Pray where is the salvation? Mr. Paget says the deaths to bitten cases before Pasteur's treatment was 16 per cent., but now it is only one in 250. The latter can be accounted for by the method I have described of including every bitten person whether the dog is mad or not. But to say that the number was 16 per 100 before Pasteurism was introduced is nothing but the sheerest impudence upon the part of Mr. Paget, for there are no statistics in existence to justify this reckless assertion. It is mere guesswork.

Your correspondent says he "does not say that no failures, and no mischances have ever occurred." No, I have before me at this moment, as already stated, 3,000 of them—all "protected" and all dead from the disease, and yet he says with his usual exaggeration, "But I do say most emphatically that thousands upon thousands of lives have been saved." He has not proved, nor can he prove, that one single life has ever been saved by this treatment. The evidence is all the other way.

The wicked torture and cruelty associated with the preparation of the hydrophobic virus is hardly fit for publication. Mr. Paget pretends to describe it, but he wisely omits its most revolting features.

I invite any of your readers who desire to know more of the subject (or indeed of any of the subjects treated in the course of this correspondence—I cannot call it "controversy"), to write to Miss Kidd, 32 Charing Cross, S.W., or to our local Branch Hon. Secretary, Miss Garnham, 38 Alstone Avenue, Cheltenham, when literature will be immediately supplied.

I am, Sir, your obedient servant,

WALTER R. HADWEN, M.D., J.P.,

President of the British Union for the Abolition  
of Vivisection.

Gloucester, January 23rd, 1911.



*From the "CHELTENHAM EXAMINER."*

### **Research Defence Society.**

Sir,—Now that I have given eight instances of the value of experiments on animals, let me sum up the letters which you have kindly published from me up to the present time. On December 8th, I gave an account of the discovery of the true cause of plague, and of the excellent results obtained by the use of the preventive treatment. On December 15th, I gave an account of the discovery of the cause of Malta fever, and how the disease had been extinguished, among our garrison in Malta, by the prohibition of the use of goats' milk, which had been found to convey the germs of the disease. On December 22nd, I took for my subject the discovery of the causes of typhoid fever and of tetanus; and the good results which had followed the preventive treatment against these two diseases. On December 29th, I gave the statistics of the serum treatment of the epidemic meningitis: the cause of the disease, and the serum treatment of this disease, were both of them discovered by experiments on animals. On January 5th, I described diphtheria-antitoxin and the results of fifteen years' experience of its use. On January 12th, I took the subject of tuberculosis, and showed what great and enduring advantages were given to the world by Koch's work. On January 19th, I took the subject of rabies, and gave an account of Pasteur's work on this disease, and of the admirable results of the Pasteur treatment.

I come now to certain statements made, by your other correspondent, on these very important subjects. The germ theory of plague, he says, "saves a lot of thinking." Haffkine's vaccine he calls a serum, and says that it has been given up as useless. He says that it increased the mortality of plague, and he calls it "an insane process of blood poisoning, and plague dissemination." With regard to Malta fever, he says that Bruce "produced symptoms of blood poisoning and called it Malta fever." He says that the cases of accidental infection with Malta fever in laboratories "appear to have been pure cases of septicaemia." He calls the work of the Royal Society's Commission "most atrocious and unscientific," and says that they got their science from a fairy-tale. He says that they now call Malta fever by the name of simple continued fever, and that our military authorities have become the laughing-stock of all the inhabitants of Malta. Of epidemic meningitis, he says that Dr. Flexner's figures are "simply fraudulent," and are "one of the most delightful specimens of impudent cooking." Of typhoid fever, he says that it results "from the actual introduction of animal organic matter, usually of an excremental type, into the alimentary canal." Of tetanus, he says that Dr. Warbasse's facts are a fairy-tale and a farrago: and that the use of tetanus-antitoxin is "filthy quackery." Of diphtheria-antitoxin, he says that the use of it is "filthy, useless, and dangerous quackery." Of tuberculosis, he says that the whole idea that it is due to the

bacillus of tubercle is an absurdity. And I do not doubt that he is this week saying the same sort of things about the preventive against rabies.

Of course, his quarrel is not with me, but with his profession. I have only stated, in my letters, the general belief of the medical profession. Your readers, if they have any doubt as to the fact of the case, must show this correspondence to any medical practitioner, or any Medical Officer of Health, or any medical student, or any hospital Sister, or well-trained nurse, in Cheltenham, Gloucester, or Clifton, or anywhere else: or to any man or woman of science who has ever studied the work of Pasteur, Lister, Koch, and their followers. The discoveries which I have named are in use all over the civilised world. They enter into daily practice: they are familiar to the whole medical profession, I can only advise your readers, if they think that I have not told the truth, to show this correspondence to their own doctor, or to any doctor anywhere in the United Kingdom.

Let me add, that in my last letter I made a mistake. The number of patients in Class B, treated in 1909 at the Pasteur Institute, was not 220, but 200.

But, I am not inclined to continue this correspondence. The tone of your correspondent's letters makes it impossible for me to take them as a serious contribution to the present controversy. It is no wonder, when the President of one of the sixteen anti-vivisection societies is capable of saying such things about the work of his own profession, that the anti-vivisection movement is fast losing hold of thoughtful, fair-minded, and well educated people.

I remain, Sir, your obedient servant,

STEPHEN PAGET,

Hon. Sec. Research Defence Society.

21 Ladbroke Square, London, W.

Jan. 23rd, 1911.

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*N.B.—Mr. Paget having thus fulfilled Dr. Hadwen's prophecy by declining "to continue this correspondence" when his series of leaflets was exhausted, and referring the public to—inter alia—"any medical student" or "any well-trained nurse," the letters came to an end. The reader is asked to reply to the question "WHO ARGUES?" and to remember that it is synonymous with "WHO THINKS?."*





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